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OBSERVATION VERSUS EXPERIMENTATION[1]

IN gatherings of scientific men such as this one, it is customary to have a number of non-technical addresses, which often take the form of general surveys of certain fields of science, with summaries of what is known in various directions, and with indications of problems which await solution. The topic which I have chosen, however, would indicate that for the moment it seems to me worth while to stop and discuss somewhat the methods of science rather than the results. No doubt all of us look upon both observation and experimentation as necessary evils, the means to arrive at ends or results which are much more important and attractive in themselves than are the processes of obtaining them.

Before a company of astronomers the contest between observation and experimentation might be anticipated to mean a discussion of the relative merits of the old and new astronomy, the astronomy of position, or of precision as its devotees often call it, and the newer field of astrophysics. Or the contest might be between the whole field of astronomy on the one side and the domain of physics and other experimental sciences on the other, for we astronomers have the reputation of being precise and painstaking observers, while the experimenters have, to our minds at least, the habit of spending most of their energies in getting ready to be precise, and then when they are prepared to take what we would call observations, their aim is achieved and they pass on to something else. But my purpose is rather to consider somewhat the struggle which often goes on in the mind of the investigator himself, whether he shall after a certain amount of

[1] Address of the retiring vice-president and chairman of Section D—Astronomy, American Association for the Advancement of Science, Toronto, December, 1921.

preparation begin observing, or whether he shall consider that his conditions are not yet favorable for exact work. Likewise the question may come up in any long series of observations: When is it better to stop and try to improve things rather than to go on in a routine? A similar choice may come to an individual even in deciding his preference for one science or another, and in the fundamental sense this same choice runs through much of our lives, the attraction of the old versus the new.

While we may take up certain considerations from the limited point of view of astronomers, there are undoubtedly applications of these same ideas in many fields of science. Of course we are all interested in improvements, and no one of us would care to admit that he has not the patience and concentration to keep at a task until he has mastered it and can do it well. There are, however, differences in individuals, and as time goes on these are accentuated, and each worker naturally tends to gravitate into the field where he works best and feels at home. The skillful observer is usually an orderly person who keeps his surroundings and apparatus neat and tidy. His instinct is to maintain constant conditions, and if his instrument or apparatus is working perfectly, to let everything remains undisturbed. There is good reason for this, since experience often shows that variation of conditions introduces unsuspected errors. The experimenter, on the other hand, seems to take delight in being surrounded by the débris of his work. Order and system are not part of his creed. He has no hesitancy in dissecting any fine new instrument if some of its pieces will fit in with what he wants, probably much to the consternation of his colleague who is responsible for the equipment. Whenever the observer sees or does anything, he writes down a note, but writing is the last thing of which the experimenter thinks. The observer takes apparatus as it comes to him, the experimenter improves apparatus or devises something new. The observer keeps all or almost all his work, the experimenter has no scruples in throwing away anything which he thinks he can improve upon.

I remember visiting a laboratory in company with a prominent astronomer, where we were shown some spectrum photographs. The physicist in charge showed us a negative which he had just taken, and then threw it aside. My companion promptly asked if there could not be something of value on that plate, if it should not be kept. The experimenter answered that he had dozens equally poor, and that he could reproduce it at will. To the observer even a poor photograph may represent an opportunity which will never return.

It is much easier to teach large classes of students to observe, after a fashion, than to experiment. In a laboratory section, the student will consider favorably a system which enables him to come in and sit down at his table, and without delay to begin and simply take readings. We hear a great deal about teaching the scientific method, but it would be quite impracticable to inflict upon elementary students the real methods of science, the trials and waste of time which any one must undergo before he can determine what he needs, and then find and assemble his apparatus.

There is one direction in which an observer sometimes feels that he has the advantage over the experimenter, and that is in this matter of waste of time. An hour's work for the observer brings an hour's results, whereas the experimenter often puts in a great deal of effort with apparently no return. A safe program of observation brings in sure returns; but is not any one mistaken in assuming that he can avoid waste of effort? It is the fate of most scientific work to be superseded, and the most accurate observations are likely to be quite out of date even in the lifetime of an individual. Bradley's star places have been and are still of great importance as a basis for proper motions of stars, but the time will come when the so-called modern observations will be of the same order of antiquity as those of a century and a half ago, and Bradley's observations will gradually lose their importance. On the other hand, there are many results from positions and proper motions of stars determined from current measures which are obviously of permanent value. Such a case is Boss's cluster in Taurus, a group of stars now widely dispersed, but which as time goes on

will gradually condense and become more and more conspicuous as an illustration of what can be predicted from precise data.

Photographic parallax determinations seem to be relegating all previous results to the discard, but parallax observers might as well hurry and get these results while they are still valuable, as the spectroscopic method though at present dependent upon the trigonometric results for a basis need not always remain so, and the mere possibility of interferometer measures of parallaxes should be enough to dampen one's enthusiasm for undertaking too large a program of safe and sane trigonometric determinations.

One of the striking differences between observers and experimenters is their use of the method of least squares. I have heard a young physicist state that he had been advised against taking a course in least squares, because he would never have occasion to use that subject in physics. The answer is that both he and his adviser have probably used the method a great deal, without being aware of it. Experimenters as a rule do not repeat measures enough to get many residuals—one astronomer has said that he wants at least fifty observations to determine a reliable probable error—but the method of least squares is by no means as limited in its usefulness as might be imagined. It is striking in how many fields of exact science the discussion of measurements takes the directions of a graphical exhibition of the results. The experimenter gets some measures which he puts on a graph exhibiting, say, the dependence of one variable upon another. Through a series of plotted points he proceeds to draw a curve; but how does he draw this curve Just what does he try to do when he makes a smooth line pass through a series of points Even for the simplest case of a straight line if you ask a student what he does, he may say that he tries to draw the line as "near as possible to all of the points," whatever they may mean, or he may try to have as many points on one side as on the other side of the line. It is very doubtful if by intuition he will draw that line which makes the sum of the squares of the residuals a minimum, and it is difficult to see how he is to fit any curve to observations without using some of the principles of the method of least squares.

In passing it might be noted that some authors still persist in publishing curves without representing the observed points on which these curves are based. Such a suppression of evidence should not be countenanced. especially as the graph of the original observations gives any one else such a convenience test of the reliability of the curves.

An application of the method of least squares which is of the utmost importance to the experimenter is in the law of propagation of error. The well known relation

$$R^2 = \left(\frac{dX}{dx_1}\right)^2 r_1^2 + \left(\frac{dX}{dx_2}\right)^2 r_2^2 + \;-\;-\;-$$

where R is the probable error of X, a function of several measured quantities, X_1, X_2, ..., is not only useful for determining the probable error of a result, but is even more important in planning a program of observation or of experimentation. Where several quantities enter into a determination there is no object in spending time or effort in the wrong place, and one wonders at the tremendous amount of misdirected effort which is constantly being wasted because of investigators measuring and being careful about the wrong thing, when an elementary acquaintance with this formula would show them which of the various sources of error was contributing most to the inaccuracy of the result. Another advantage of the method of least squares is that it enables a number of unknown quantities to be disentangled from a mass of data where it has been impossible for the experimenter to differentiate with respect to one variable at a time. In astronomical practice this is too elementary even to mention, but it is amazing how physicists and others can get along without knowing how to proceed when the conditions are such that they can make only indirect observations on several quantities. It is, of course, the safest practice to measure directly the quantity sought, and to vary but one thing at a time when that is possible, but an experimenter may find advantage in knowing how to derive several unknowns simultaneously.

However, with all of the advantages of the method of least squares, it is not so seldom

that its devotees may go too far with it. How often it occurs that the accuracy of a series of measures as indicated by the probable error is illusory In almost every field of exact measurement we have the presence of both accidental and systematic errors, and he is an optimist indeed who deals with only the former. It is here that the experimenter is at an advantage, as he naturally is constantly seeking to eliminate undesirable factors, and by constantly changing conditions may vary or eliminate what may be called the systematic errors.

It has been said that a worker in exact science usually goes through three stages of attitude toward his work. He starts out by considering every small or unexpected discrepancy as due to a physical reality; after being deceived a sufficient number of times, he has a reaction, and nothing is proved until it is really proved; he then gradually grows back into a state where he is neither too exultant at the first prospect of a discovery, nor too pessimistic over the insufficiency of the evidence for a result which he hopes to establish. We may quote from Langley, who in the discussion of small irregularities of his bolometer records of the solar spectrum said, "When we approach the limits of vision or audition, or of perception by any other of the human senses, no matter how these may be fortified by instrumental aid, we finally perceive, and always must perceive a condition, a condition still beyond, where certitude becomes incertitude, although we may not be able to designate precisely where one ceases and the other begins. This is always the case, it would seem, on the boundaries of our knowledge in every department, and it is so here."

In the estimate of the precision of a given result there is not yet adherence to the logical use of the probable error as a measure of precision or accordance; astronomers long ago adopted this usage, but others seem to get along without it. Only recently I heard in a public address the statement that a certain measure could be made "with an error of one part in a thousand." Just what was meant by this would be difficult to determine, especially as the speaker afterwards said that the "range did not exceed one part in a thousand." These loose statements did not come from a beginner but from a master in the art of exact measurement. Still another example is found in a recent number of a standard journal: "The maximum error is .1 per cent." This is presumably some sort of estimate of the possible systematic error of the result, but one would think that physicists would come to some common ground in describing their errors, so that they could understand each other. One suspects that here we have simply an illustration of the difference between the observer and the experimenter; the former stays with his measures long enough to have a real basis for computing a probable error, the latter has a few measures, and even if he used the formula for the probable error he would be doubtful of its value. Experimenters boast when they have achieved "astronomical precision" in the number of significant figures in their results, but they might equally well cultivate some astronomical accuracy of statement when it comes to describing the reliability or accordance of their results.

The term "astronomical" precision brings to mind the prediction of some years ago that most new discoveries in physics would be in the sixth place of decimals. Whatever else may be said concerning the advances in that science, it will not be maintained that so many significant figures have been necessary to establish the important results. Intelligent lay opinion might be somewhat shocked to learn by what methods astronomers are measuring or estimating distances of stars. A mere guess at the mass of a stellar system may give its distance with far greater accuracy than could possibly be secured by the method of exact measurement. The new things in science continue to be not in the last place but often in the first place of decimals. We should be quite happy to have one significant figure correct in a measure of the size of the visible universe.

There is one particular field in astronomy where the technique of observing as at present practiced is a constant reminder to the observer that either he or some one else had better do some experimenting, and that is in astronomical photography. Many an observer during the tedious hours of long exposure

must have felt that some of his time might better be devoted to increasing the sensitivity of the photographic plate, rather than to be continuing the drudgery of keeping a telescope accurately on a star for hours at a time. However, the astronomer knows well that the plate makers themselves are fully alive to the desirability of faster plates, which would have such an enormous commercial value that the astronomical applications would seem trivial in comparison. Nevertheless, one can not but speculate on the field which would be opened to small telescopes if the photographic plate were increased say tenfold in sensitivity, not to mention the power which would then come to large instruments.

There is little need of discussing the relative advantages of large and small telescopes, one might as well discuss the possibilities of abundant and meager resources; but there is at least the consolation to a possessor of a small instrument that he does not need to use it all the time simply to justify the capital expenditure in his equipment. He is therefore much freer to try out new ideas, and even to waste a great deal of time, without the immediate necessity of producing results in proportion to his facilities. The large and well equipped institutions have by no means a monopoly on revolutionary improvements or discoveries.

The choice of an individual between joining a large or a small institution may or may not be the same as the choice between observation and experimentation. In some large places he may become simply a cog in the machine, and easily sink into a narrow routine. On the other hand, the resources of a large place may make it possible for him to try out various schemes which would be quite impossible if he were off by himself. On the whole, one must balance the advantages of each type of institution, but he is a fortunate individual if he has free choice in which direction he will work. There is one resource, however, which is necessary to all scientific investigation, and this is the item of time. You may deprive the investigator of much of his physical equipment and resources, and with plenty of free time he can go on, almost with bare hands as it were; but take away the opportunity to make continued effort, and he will cease to produce. As an illustration of what may be done with almost no equipment we may cite the case of the late Simon Newcomb, who while visiting at a summer resort made a determination of the fundamental quantity, the total light of all the stars. His apparatus comprised only several spectacle lenses, but he succeeded in obtaining a result, and any possessor of a large telescope would be satisfied if he could with all his means occasionally produce something as valuable as that work of Professor Newcomb.

But after all, both the experimenter and observer need to discuss their work, and this entails a certain amount of computation. As a rule the observer becomes more adept in the art of computation simply because he has more of it to do, but either observer or experimenter will probably look upon long computations simply as necessary evils. It has been said of a certain astronomer that his dream of heaven is a sky full of comets and a room full of computers to work out their orbits for him. This reminds us that most important of all is theorization; all of the routine of scientific work, experimentation, observation, and computation are simply a means to an end. The real joy consists in sitting at one's desk and making discoveries which come out of previous work, either from one's own or from that of others. Perhaps the ideal case is where a single individual is able to partake in all phases of investigation, from the preliminary securing of data up to the final discussion of the theoretical bearing of the results. In the old days this was more easy to do than now, for as science becomes more and more complex it is increasingly difficult for one person to master the technique of all the processes involved in a single problem, and with the growth of cooperative research it is possible for several workers to join hands and accomplish what would be far beyond the powers of any one of them. But in any cooperative scheme it should be borne in mind that what is wanted is real cooperation on a democratic basis, and not a direction of individuals by a so-called master mind. Efficient as an autocratic system may be, in science as in other fields it ultimately

fails in the question of morale, for when young scientific workers see that however attractive may be the places of the men at the top, the chances for any individual are that he will become only a part of an efficient machine, then a man of ambition will choose some machine where the material rewards are greater than in science.

One great disadvantage in the arrangement of separating the observer and the computer is that a realization of attainable accuracy is likely to be lost. It sometimes seems that the farther the computer is removed in time and place from the original observations, the greater is the accuracy which these observations take on. A good illustration is in some modern computations of results based upon old observations of variable stars. The method of Argelander, of simply looking first at one star and then at another, and estimating the difference of brightness, is still of the utmost value, but errors as great as ten or twenty per cent, in the ratio of the light of two stars are not uncommon. We can make the accuracy seem greater by expressing the estimate in stellar magnitude, when the errors are only one or two tenths of a magnitude, but the fact remains that the discordances are a large fraction of the quantities sought. Some computers taking results of such estimates have managed to derive elements of variable stars where some of the derived quantities are expressed to five significant figures, although the original data were often wrong in the second figure. This fictitious accuracy seems to come from a state of mind where the more you compute the more figures you get, and the investigator needs the restraining influence of experience in securing observational data. Of course, the computer, if he goes about it in the right way, can really show the observer just how accurate the measures are, but in his anxiety to establish some fine theory the computer sometimes loses his own sense of proportion.

And so it goes; the observer does not know how to observe unless he realizes the value of experiment; the experimenter loses a great deal if he has not acquired the technique of observation; neither the experimenter nor the observer can work to the best advantage unless he has the proper theoretical background; and the pure theorist may be saved from various grotesque mistakes if he becomes acquainted with some of the methods and difficulties of securing the facts of physical science.

We may, therefore, best dwell not on the differences among experimenters, observers, and theorists, but rather on their strength when united and working together. No matter how well rounded an individual may become, his capabilities may be easily surpassed by a group of cooperating workers. If it be objected that new ideas will not originate in a committee, the answer is that any one of us has plenty of ideas, many of them fundamental and important, but what we lack is the ability and power to put our ideas into execution. It is here that to my mind lies the great advantage of the policy of the National Research Council in bringing together in committee workers from all over the country so that they can form plans of joint attack on various problems. In our universities and other institutions there is great opportunity for cooperative effort between colleagues, but even in the same institution or department the interests may be so divergent that a worker may find little help of just the kind that he needs, whereas in some other parts of the country may be one or more competitors who, if they can be got together to talk things over, will turn out to be only hearty collaborators.

Astronomy is called the oldest of the sciences; our friends in other fields say that it has been in the lead in America, and especially that astronomers were the first to organize cooperation in research. Let us not fail to continue to deserve this good name, and to set the example in so far as we can of free trade and mutual good will in the solving of our problems.

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GENERAL FEATURES OF THE TORONTO MEETING

THE second Toronto meeting of the American Association for the Advancement of Science and of the associated scientific societies,